Explanation*

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1. Introduction

In what sense do the sciences explain? Or do they merely describe what is going on without answering why-questions at all. But cannot description at an appropriate 'level' provide all that we can reasonably ask of an explanation? Well, what do we mean by explanation anyway? What, if anything, gets left out when we provide a so-called scientific explanation? Are there limits of explanation in general, and scientific explanation, in particular? What are the criteria for a *good* explanation? Is it possible to satisfy all the desiderata simultaneously? If not, which should we regard as paramount? What is the connection between explanation and prediction? What exactly is it that statistical explanations explain? These are some of the questions that have generated a very extensive literature in the philosophy of science. In attempting to answer them, definite views will have to be taken on related matters, such as physical laws, causality, reduction, and questions of evidence and confirmation, of theory and observation, realism versus anti-realism, and the objectivity and rationality of science. I will state my own views on these matters, in the course of this essay. To argue for everything in detail and to do justice to all the alternative views, would fill a book, perhaps several books. I want to lead up fairly quickly to modern physics, and review the explanatory situation there in rather more detail.

2. Why-questions: the D-N model as providing a necessary condition for explanation

'Why' is ambiguous.[1] If someone asserts that the earth is round, and I ask 'Why', I may be asking for a reason why you believe that the earth is round. What is your evidence for the assertion? Typically you may refer to *consequences* of the earth being round, horizon phenomena, the shape of the shadow of the earth on the face of the moon during an

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[1] Cf. Hospers (1967), 240.

eclipse, and so on. The evidence you cite supports or confirms the hypothesis that the earth is round, justifies you in believing that the earth is round. But I may also be asking for an explanation: why *is* the earth round? Is it just a brute fact or can you *explain* it, in a way which confers understanding and removes my perplexity in the face of just reciting the brute fact? Typically you will cite the roundess of the earth as itself a *consequence* of other propositions in a manner which contributes to my understanding. This is the famous deductive model of explanation.[2] Is deducibility either sufficient or necessary for a scientific explanation? Well, it is obviously not sufficient. Most trivially X is deducible from X, and we can hardly allow X to explain itself. But I am going to claim that it is necessary. This is to fly in the face of most of the post-Hempelian literature on explanation.[3] There are two sorts of counter-examples to necessity commonly cited. First, volcanoes—we cannot deduce when they will erupt, but, so it is said, we can explain an eruption *post hoc* in terms of known geological process. But that is to confuse an explanation-sketch with a full-blooded scientific explanation.[4] If we knew enough about the distribution of stress in the rocks and the laws governing mechanical rupture we could make the prediction. If we do not possess all the relevant information then we are not in a position to give a scientific explanation, that is full or complete. That is not to say that partial explanations may not confer a measure of understanding, but they do not measure up to the scientific ideal involving strict deducibility, i.e., in the volcano example, predictability.

There is an important ambiguity here we must be clear about. Many physical systems are governed by deterministic laws in the sense that *exact* specification of initial and boundary conditions fixes the later physical state uniquely, but the prediction is unstable in the sense that any error, *however small*, introduces divergent behaviour in specifying the future state of the system. So, in practice, and even in principle, *we* cannot compute the prediction. But from a 'God's eye' point of view everything is fixed. In this paper we are not concerning ourselves primarily with the pragmatics of explanation, and hence when we talk of predictability, we mean this in the ontological 'God's eye' sense.

But some events cannot be predicted. Does this mean they cannot be explained? I think the appropriate answer to this question is to bite the

[2] The classic statement of the D-N model of explanation is Hempel and Oppenheim (1948)—reprinted in Hempel (1965).

[3] Influential critiques of the Hempelian approach include Scriven (1962) and Salmon (1971). A comprehensive survey of the literature on explanation is provided by Achinstein (1983).

[4] Cf. Hempel (1965), 416.

bullet and say 'yes'. So what do probabilisitic or statistical explanations achieve? Well, they enable us to deduce and hence to explain the limiting relative frequencies with which events of a given kind turn up in a long-run repetition of the set-up producing the phenomenon. But they cannot explain what happens on a particular occasion. It is as simple as that!

In particular they cannot explain in the strict deductivist sense the relative proportions in a *finite* sample. Suffice it to say that these proportions can be used to provide a rational estimation of what can be explained, i.e. the probabilities, in accordance with the usual statistical procedures.

There is an enormous literature on statistical explanation, arguing whether the probability of the explanandum has to be high in the presence of the explanans[5] or merely higher than it would be in the absence of the explanans.[6] In the strict *scientific* sense, I regard these discussions as irrelevant. Again I am not denying that there are senses of promoting understanding, or removing perplexity, other than that produced by the scientific ideal. But it is with the scientific ideal that we are concerned.

But if deducibility is admitted to be necessary for scientific explanation, how are we to fill out the conditions to achieve sufficiency. That is a much more difficult task, to which I now turn.

3. The Circularity Objection

In the deductive-nomological (D-N) model of Hempel the explanans cites one or more scientific laws. In the usual schematic fashion adopted by philosophers of science, let us represent a typical scientific law in the universally quantified form $\forall x(Px \rightarrow Qx)$—succinctly all Ps are Qs.

If *a* is a P, i.e. Pa is true, then we seek to explain why *a* is a Q by deducing Qa from the premisses

$$\forall x(Px \rightarrow Qx) \tag{1}$$
$$Pa \tag{2}$$

Thus: from (1) by Universal Instantiation

$$Pa \rightarrow Qa \tag{3}$$

Whence, from (2) and (3) by *modus ponens*

$$Qa$$

Why is this thought to be explanatory? In (1) the implication as we have written it is material implication. On a Humean (regularity) view of

[5] See Hempel (1965), 376ff.
[6] Cf. Salmon (1971).

laws that is all there is to (1).[7] It is true in virtue of all its instances being true. But if (1) depends for its truth on the truth of (3), and this, given the premiss Pa, must turn on the truth Qa. So is not the argument completely circular? The truth of Qa, given Pa, is grounded in the truth of a universal stratement, whose truth is grounded in the truth of Qa, the very fact we are trying to explain. What this amounts to is that (1) is nothing more nor less, on the Humean account, than a compendium of all the instances (3). (In the case of a finite variety of instances the universal law is indeed nothing else than the conjunction of its instances.) On the Humean account the instances are 'loose' (there is no cement!) so effectively the Hempelian model, under this interpretation of law, amounts to the assertion that facts only explain themselves.

But this whole argument hinges on the assumption that the universal law is only supported or confirmed by the totality of evidence which would make the law (deductively) true! If we think that (1) may be supported in some *inductive* sense by instances other than (3) then the circularity would be satisfactorily mitigated. But here we are backing ourselves straight into the problem of induction. That induction is not *deductively* valid as a mode of inference, committing as it does the fallacy of affirming the consequent, must of course be admitted. So in our example, if we are to avoid circularity, we must acknowledge that the explanans is never known definitely to be true, and this is an obvious, but unavoidable, defect in scientific explanations. For a Popperian, of course, the matter is much worse than that. Finite evidence never supports to any degree the conjectural truth of universal laws quantified over infinite domains. So a Popperian expects, in so far as he allows himself any expectations, that an essential part of the explanans, in any scientific explanation, is definitely false (although not currently known to be false!). Does this render scientific explanations irrational?—a conclusion much advertised by the critics of Popperism. The answer depends very much on what we count as rational. Is it rational to aim at the impossible? Can we not rationally *accept* scientific laws, without believing them to be true, provided they have been subject to the severest available *criticism*?[8]

Let us turn from these deep methodological issues to consider whether a necessitarian account of natural laws helps with the question of explanation. On the question of evidence not at all. After all, that was

[7] The Humean view of laws is most recently defended in Swartz (1985). More sophisticated Humeans of the Ramsey-Lewis stripe incorporates a 'more for less' criterion for natural laws that links closely with our later discussion of Friedman's views on explanation and unification. A good exposition of the Ramsey-Lewis approach is provided in Armstrong (1983), chapter 5.

[8] For a sustained defence of neo-Popperian rationality see Watkins (1984).

Hume's original point. We have no epistemological access to the idea of necessary connection. But let us take an ontological standpoint. If laws of nature involve nomological necessity, however analysed, over and above the merely factual material implication, would this not account for how Pa being the case is the ground for Qa being the case? The ground for Qa being grounded in Pa is the necessary connection between P'ness and Q'ness on one popular understanding of these matters.[9] Of course, we can press on to query the ground for *that* ground. We shall return to the question of ultimate or self-supporting explanations presently. But there are other matters we want to deal with.

Firstly, superfluous aspects of explanation: we deduced Qa from (1) and (2), but we could also deduce it from (1), (2) and

God's in his Heaven (4)

We must check that (4) is idle, that we could equally deduce Qa from (1), (2) and the negation of (4).

Then there is the question of explanations which could be correct but as a matter of fact are not. Example: Fred who is shot through the head, after he has died as a result of being stabbed through the heart! The shot could have been the correct explanation of his death, but in deciding whether it actually is, we need to attend to all the relevant circumstances. The law linking shooting through the head with death is more accurately rendered as linking the shot with a transition from life to death. As Fred is already dead, this more fully amplified version of the law is no longer applicable and cannot be cited in explaining Fred's death.[10] Again the scientific ideal assumes that all the relevant circumstances are being cited.

So far we have given some indication of what constitutes an explanation in science, but what constitutes a *good* explanation, when is one explanation better than another?

4. Good Explanations: Unification

There is first of all the element of suprise, of unexpectedness, the 'Aha' factor.[11] That is no doubt related to the criterion of non-circularity, that the explanandum in no way presents itself as what we take to be the

[9] Cf. Armstrong (1983).

[10] For a critical discussion of whether *ceteris paribus* clauses and provisos can in principle or in practice be spelled out in the required detail see Grünbaum and Salmon (1988).

[11] See Feigl (1949).

evidence for the explanans. In practice good explanations, by this dimension of appraisal, arise at the intersection of several universal laws, all of which are necessary to deduce the explanandum.[12] Iron sinks in water, not just because all solids with density greater than that of water sink in water, or even with density greater than that of liquid in which it is placed, but better, because the difference in density is associated via Archimedes Principle with an imbalance of weight versus buoyancy, which shows, from the laws of mechanics, the direction in which the iron will move. The sinking of the iron is not just a special case of a more inclusive law, and so would not be cited as direct evidence for any of the laws stated separately.

But there is another important aspect that this example brings out. What we described as the better explanation involves a series of laws, which can be used to deduce and hence explain many other facts in the field of hydrostatics or with appropriate extension of the theoretical apparatus, the enormous richness of hydrodynamic phenomena from waterfalls to the breaking of waves on the seashore, from whirlpools to the lift and drag of an aerofoil. This points to the very important unification aspect of explanation. The world at the surface level of immediate experience appears very complicated, very rich in diverse phenomena with no apparent connection. But at a 'deeper' theoretical level can all this diversity get reduced to a few interlocking explanatory principles? This has always provided an ideal of theoretical progress in science, the ideal of unification. There is no doubt that the history of modern physics has provided examples of increasing unification in our fundamental theories. But it is important to be clear as to what is being claimed. There are a number of distinct senses of unification that need to be distinguished.[13]

Firstly, there is the question of the inter-relatedness, or 'working together' of the explanatory nexus. Supose we have two sorts of phenomena, P_1 and P_2, which stand for the sets of lawlike regularity governing the phenomena (at the immediate 'empirical' level of everyday physicalist discourse) and suppose that P_1 and P_2 are explained by theories T_1 and T_2. Then $P_1 \cup P_2$ is certainly explained by $T_1 \wedge T_2$ since any member p_1 of P_1 can be deduced from T_1 and any member p_2 of P_2 can be deduced from T_2. In a trivial sense there are new predictions that can be deduced from $T_1 \wedge T_2$ but not from either theory separately, viz. conjunctions like $p_1 \wedge p_2$. But there are no *interesting* novel predictions that arise from $T_1 \wedge T_2$. In one sense of unification, a unified explanation of $P_1 \cup P_2$ would arise from a theory T in which there was no partition of the axioms which separately yielded all the (interesting)

[12] See Nagel (1961), 34ff.

[13] Cf. Redhead (1984).

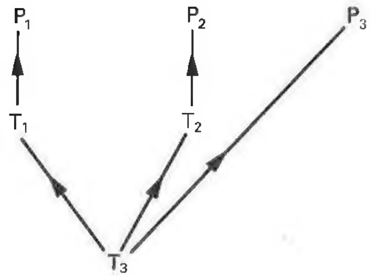

Figure 1

predictions. There would be predictions that required the interlocking working together of the axioms. Typically a unified explanation T of $P_1 \cup P_2$ would also predict new phenomena arising from the interactive effect of the axioms comprising T. Such an explanation would not only be unified but could be in a sense 'deeper' than T_1 and T_2. Suppose we denote the new phenomena by P_3, then we can illustrate the situation we have in mind very schematically as shown in Figure 1. T_3 not only explains T_1 and T_2 that originally accounted for P_1 and P_2, but makes new prediction P_3 and this is done in such a way that T_3 provides a unified account of $P_1 \cup P_2 \cup P_3$. There are many complications associated with working out this idea of unity-cum-depth. These are admirably treated in Watkins' 1984-monograph *Science and Scepticism*. Firstly T_3 may correct T_1 and T_2, not just unify them, and then the idea of increased empirical content becomes formally problematic. Watkins deals with this by his method of counterparts in which incompatible statements are 'matched up' according to appropriate rules. Then there is the problem that a theory such as T_3 which may be unified under one axiomatization may become non-unified under an alternative axiomatization. To deal with this problem Watkins resorts to a notion of 'natural axiomatization', satisfying rules that prevent unnecessary proliferation of axioms and defeat proposed unification-defeating reaxiomatizations. In addition to independence and non-redundancy requirements, Watkins has a rule demanding segregation of axioms containing only theoretical terms, but more importantly there is a non-molecularity requirement, stating that an axiom is impermissible if it contains a proper component which is a theorem of the axiom set, or becomes one when its variables are bound by the quantifiers that bind them in the axiom. Finally there is a decomposition requirement specifying that if the axiom set can be replaced by an equivalent one that is more numerous, without violating the other rules, it should be.

I want to use Watkins' idea of a natural axiomatization to solve a vexing problem that now arises. Why does T_3 remove our perplexity about $P_1 \cup P_2 \cup P_3$? Part of the answer lies in showing that these

apparently unconnected phenomena are in fact related via the unified derivation from T_3 (under a natural axiomatization). But we also want to say that T_3 is in some intuitive sense simpler than $T_1 \wedge T_2$ together with P_3 itself. An obvious approach here is just to count the number of axioms $N_A(T_3)$ in a natural axiomatization of T_3 and check whether this is less than $N_A(T_1)+N_A(T_2)+N_A(P_3)$. (Note that Watkins assumes the underlying logic and mathematics is already 'given', so we are concerned with a *finite* number of non-logical and non-mathematical axioms. These may be regarded as part of the specification of a class of models in the Sneed-Stegmüller structuralist approach to theories, although Watkins himself seems to subscribe to the standard 'statement' view of theories'.) What do we mean by $N_A(P_3)$? Well it is the number of axioms in a natural axiomatization of the phenomena comprised in P_3. This in effect means counting the number of laws in P_3. But we must be careful that the laws are expressed in a manner allowed by Watkins' rules. For example, as Friedman (1974) noted, any statement Q can be expressed as the conjunction of N statements, for any N. Thus write

$$
\begin{aligned}
Q &\equiv P_1 \wedge (P_1 \rightarrow Q) \\
&\equiv P_2 \wedge (P_2 \rightarrow P_1) \wedge (P_1 \rightarrow Q) \\
&\overline{\phantom{\equiv P_{n-1} \wedge (P_{n-1} \rightarrow P_{n-2}) \wedge (P_{n-2} \rightarrow P_{n-3}) \ldots \wedge (P_1 \rightarrow Q)}} \\
&\equiv P_{n-1} \wedge (P_{n-1} \rightarrow P_{n-2}) \wedge (P_{n-2} \rightarrow P_{n-3}) \ldots \wedge (P_1 \rightarrow Q)
\end{aligned}
$$

Where $P_1, P_2 \ldots P_{n-1}$ is a descending chain of increasingly weak logical consequences of the statement Q. Such rewritings for law statements Q would be eliminated immediately by Watkins' rules against inessential proliferation of axioms.

We can also compare $N_A(T_1)$ with $N_A(P_1)$ and $N_A(T_2)$ with $N_A(P_2)$ to see how a reduction in the number of laws left unexplained may already have been achieved at the level of introducing T_1 and T_2, and compare with the further reduction effected by T_3.

On this account the reduction in our perplexity in the face of P_1, P_2, P_3 corresponds just to the reduction to $N_A(T_3)$ of those laws of nature which we have to accept without explanation. This was the intention behind Friedman's (1974) approach to explanation, but the account he gave was technically quite incoherent, as shown by Kitcher (1976). Kitcher himself rejected the simple idea of counting laws in an explanatory framework in favour of (effectively) counting what he calls patterns of explanation (see Kitcher (1981) for details). I believe myself that the original Friedman approach is much more straightforward and perspicuous, if it can be rescued from the Kitcherian strictures by employing the Watkins natural axiomatization approach in the way I have described.

There is one important matter we must attend to. We have spoken of laws of nature comprised in the axioms of T_3 explaining laws of nature comprised in the phenomena P_1, P_2 and P_3. But we must be clear as to what we are going to allow as a law of nature. It expresses a regularity, that all items of a certain kind, the subject S of the law, subject to certain conditions C, behave according to some property P. The problem in counting laws is concerned with the question of how general S needs to be and how specific C. If we allow for the maximum generality in S and the minimum specificity in C we could easily rule out electromagnetic laws, for example, other than: all charged bodies obey Maxwell's equations! Achinstein in his (1971) argues that we may refer to a universal generalization as a law if at some period in the history of science it is not known (or believed) that generalization is possible, to a more inclusive law. But this approach introduces what many philosophers regard as an unacceptable historical relativism in place of an objective criterion. Perhaps we should just accept that all generalizations that can be deduced as theorems in an axiomatic-deductive framework should count as laws. The question at issue is also related to the vexed notion of natural kinds, that the extension of the subject-predicate in a law should not be the result of some arbitrary and conventionally imposed system of classification, that the subject-predicate should correspond to a 'genuine' universal. At all events, while there may be borderline cases, such as the much-discussed example of Kepler's laws, where the decision in these matters is somewhat controversial, I believe that sufficient liberality in identifying generalization as laws will permit the notion of unification to be a useful and important one in discussions of the scientific explanation of laws. The essential point is that unification delimits what we must accept without explanation.

In many discussions of explanation the point is made that the explanans must comprise some distinguished set of explanatory principles. On some accounts[14] the explanans must have an analogy with phenomena with which we are familiar in everyday experience, that perplexity in the face of the explanandum is removed by exploiting our familiarity with the analogy. Crudely we may require the explanans to be in some sense picturable. This requirement, while clearly exemplified in examples such as the nineteenth century mechanical aether models, is not characteristic of explanations in modern theoretical physics. While pictures, such as the Feynman diagrams in elementary particle physics, are an aid to keeping track of complicated computational procedures, they are actually potentially very mislead-

[14] See Campbell (1920) and Hesse (1966).

ing in affording what we may term physical understanding of what is going on.[15]

On other accounts the privileged set of explanatory principles is relativized to what in a particular theoretical-conceptual scheme are regarded as 'natural', as not requiring explanation. We shall return to this requirement in a moment in discussing the relevance of causality to questions of explanation. But for the moment, we would merely note that the superiority of the unification approach of Friedman is that an arguably objective criterion can be presented that characterizes progress in the explanatory endeavour in science.

One of the most favoured approaches to unification in modern physics has been through the process of micro-reduction. This raises a number of issues which we now turn to.

5. Micro-reduction

The micro-reduction programme sees the microscopic world as composed of unobservable 'atomic' units whose properties serve to explain in a unified way the whole range of macroscopic phenomena. Unification now incorporates the *additional* requirement that the explanatory principles only cite properties of the microscopic entities involved in the explanans.

The question of the terminus of explanation now presents itself as the alternative between ultimate atoms and an infinite regress of 'Chinese boxes', in which micro-entities are themselves resolved into micro-micro constituents *ad infinitum.* Molecules are resolved into atoms, atoms into electrons and atomic nuclei, nuclei into protons and neutrons, nucleons into quarks . . . One must not think that micro-reduction necessarily eliminates the autonomous branches of macroscopic physics, in favour of a physics of elementary particles. The identity statements characteristic of scientific reductions may best be understood as contingent, but law-like coextensionalities. One can imagine worlds where hot bodies exist, but temperature is not a measure of mean energy of the constituent molecules. That may not be our world, but it is a possible world, whose very conceivability shows that the identity statements in reduction schemes are not analytic in character.[16]

While it is true that the major trend in modern theoretical physics has been micro-reductive, there are some caveats that must be entered.

(1) On the orthodox Copenhagen interpretation of quantum mechanics, the macroscopic world of classically described apparatus and

[15] See Redhead (1988a) for further discussion of this point.

[16] The view expressed here should be contrasted with that canvassed by Causey (1977).

experimental set-ups must be presupposed in analysing the properties of those very micro-entities which make up or constitute the macroscopic objects. In a sense the reduction instead of descending linearly towards the elementary particles, moves in a circle, linking the reductive basis back to the higher levels.[17]

(2) Even on non-orthodox interpretations of quantum mechanics of the hidden-variable variety, there is a choice between non-local action-at-a-distance between elementary particles and a holistic conception of non-separability, that in so-called entangled states it does not make sense to attribute properties to separate 'particular' entities, independently of a prior understanding of the properties of the whole composite system.[18]

(3) In resolving the problem of measurement in quantum mechanics some have held that human consciousness plays an essential role in resolving the ambiguity in 'pointer readings' predicted by straightforward application of the quantum-mechanical formalism. This would again make the reductive hierarchy Psychology → Biology → Chemistry → Physics → Elementary Particles bite at its own tail. We shall return to the question of human consciousness in scientific explanations shortly in discussing so-called anthropic explanations.

(4) In the bootstrap approach to elementary particle physics that was very popular in the 1960s, a democratic approach to composite entities, constituent entities and 'force-carrying' entities was proposed. Every entity could potentially play any of these three roles, so that the analysis of composite entities into a few 'aristocratic' constituents in the typically micro-reductive fashion was rejected.[19]

Despite these reservations, micro-reduction still seems a viable option, if taken as in point (2) with unorthodox interpretations that allow some sort of action-at-a-distance between micro-entities. This poses potential problems in reconciling quantum mechanics with special relativity. But perhaps only if the action-at-a-distance is of a causal character. This is the first point at which we have mentioned the notion of cause, and we want now to say something about the view that the only 'genuine' explanations of events occurring in the physical world are causal explanations.

6. Causal Explanations

What are causes? There is no consensus among philosophers, although they generally regard them as an important matter in any metaphysical

[17] For further discussion see Redhead (1987a).

[18] Cp. Redhead (1987b).

[19] For details see Redhead (1980a).

of deep explanatory principles that are themselves accepted, for the time being, without explanation. But of course the ideal of scientific explanation is one for ongoing improvement. Perhaps from the fundamental laws of microphysics, by some consistency criterion, it will turn out that the constants of nature are tightly constrained or even uniquely determined. But even then we would still have the task of explaining the laws themselves at a still more fundamental level. At some stage scientific explanations always turns into description—'That's how it is folks'—there is no *ultimate* terminus in science for the awkward child who persists in asking why! I do not believe the aim of some self-vindicating *à priori* foundation for science is a credible one.

Then there is the question of delimiting contingency in the world. On a necessitarian view of laws, contingency is usually understood in terms of the contrasted non-necessity of initial and boundary conditions. But in matters of cosmology that sort of distinction may not be the right one to draw. The no-boundary universe of Hawking and Hartle is an example, where essentially there are only laws![25]

Another matter deserving of mention is the role of symmetry principles, which are really metalaws constraining the form of laws themselves.[26] Many explanations in modern physics bypass the detailed laws by invoking directly the metalaws. But their status again is not self-vindicating. The discovery of the violation of mirror symmetry in the physics of weak interactions was one of the crowning triumphs of modern science.

Some people have argued that the orthodox ideal we have been expounding and defending should be given up, and replaced by an account which lies closer to scientific practice.[27] Now, it is quite true that the fundamental laws of microphysics are quite useless as a practical basis for deducing, and hence explaining many phenomena. The fundamental theories are mathematically much too intractable. So, in practice, physicists use all kinds of approximate and indeed inconsistent 'models', to discuss the properties of complicated systems such as atomic nuclei or lasers.[28] But I have been concerned with the scientific ideal. The fact that, for practical reasons, it cannot be implemented, does not detract from its status as an ideal, something we should try for, rather than substituting the messy 'real' physics as something which, instead of falling short of the ideal standard, should itself be elevated into the standard for scientific explanation.

[25] See Hawking (1988) for a popular exposition.
[26] See Redhead (1975).
[27] Cf. Cartwright (1983).
[28] See Redhead (1980b).

In his book *The View from Nowhere* (1986), Tom Nagel has argued that science in eschewing subjectivity, does not tell the whole story. To give a proper account of human actions and intentions we need the subjective view, the view from somewhere. But in its proper place and sphere the traditional methods of objective science have proved extraordinarily successful—there is no reason in my view to think that modern micro-reductive physics throws inevitable doubt on those methods and their motivating ideal of the progressive unification of science.

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